

**Peer Review of the Scientific Basis for the Klamath River Temperature, Dissolved Oxygen, Organic Matter and Nutrient TMDL**

*done at the request of the*

California Regional Water Quality Control Board

*submitted by*

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Reviewer Background

The reviewer is a faculty member in the Department of Environmental Sciences and Engineering at the University of North Carolina at Chapel Hill. My research interests involve modeling and field/experimental work in the areas of both water quality and water quantity. Most of my work related to water quality has focused on the issue of stormwater runoff and its impacts on receiving waters, with primary attention to microbial contaminants. I have worked on water quality projects in North Carolina, Texas and New York, and am currently the Principal Investigator on a project funded by the North Carolina Department of Environment and Natural Resources to develop a watershed restoration plan for an impaired waterbody (Northeast Creek) as part of the state's TMDL process. While I have experience in both water quality modeling and monitoring, I should mention that I have worked primarily on microbial and heavy metal contaminants, and not specifically on issues related to temperature, dissolved oxygen, organic matter or nutrients, so my comments should be read with this in mind.

Review Summary

The materials provided for this review include: (i) a Peer Review Draft of the Staff Report for the Klamath River Basin Temperature, Dissolved Oxygen, Organic Matter and Nutrient Total Maximum Daily Loads, dated December 2008, and (ii) Appendices 1-5 associated with the same document. Note that in the case of (i) this included only Chapters 1-5, and while it appears that Chapters 6-7 exist, they seem to have been outside of this part of the review process.

Chapters 1-2 of document (i) provide a comprehensive description of the Klamath River Basin, with detailed discussion of the physical, hydrologic and social factors at play in the region. Chapters 3-5 focus on the analytical approach, which primarily involves water quality modeling, as well as a discussion of modeling results that then serve as the primary means of determining the pollutant allocations and targets within the Basin. These allocations and targets are intended to improve water quality to the degree that the beneficial uses of the River will no longer be impaired, thus fulfilling the requirements of the TMDL process.

After reviewing these documents, my overall opinion is that the plan makes use of contemporary mechanistic water quality models that are based on sound scientific principles, and that the (largely) deterministic results appear to be reasonable given the data and information available. That said, my primary concern is that even state-of-the-art water quality models parameterized with extensive datasets are not terribly accurate, and are often unable to predict contaminant concentrations or loadings with what most would consider to be a reasonable level of accuracy. This shortcoming is certainly apparent throughout the peer-reviewed literature (e.g., Dorner et al. 2006; Reckhow 2003; Stow et al. 2003) and was a central theme in the National Research Council's 2001 report, "Assessing the TMDL Approach to Water Quality Management," which recommends explicit treatment and discussion of uncertainty as a part of the TMDL process. Consequently, reliance on deterministic modeling results without giving due attention to the (often substantial) levels of uncertainty attendant with these estimates can provide an incomplete picture to those seeking to interpret these analyses for decisionmaking purposes. While I understand that there will never be enough data to fully characterize a complex natural system such as the Klamath, and that decisions of this kind must often be made without the benefit of complete information, characterizations of the nature and importance of gaps in data and understanding should be more explicit.

Therefore, my primary suggestion would be that a more concerted effort be directed toward the evaluation and communication of the uncertainty inherent in these models. General comments related to this issue are provided below. Also included are sections that address the specific questions posed in the request for review.

### General Comments

Within the review documents, many of the determinations regarding the degree of allowable contaminant loading and the sources of that loading are made on the basis of comparisons between model estimates of "natural" background levels (made mostly without data) and model estimates of current conditions (some of which are made with the benefit of a calibration step involving current data). As such, it seems appropriate that a greater level of effort be taken to more clearly describe the degree of uncertainty attendant with both of these estimates. This will provide a better understanding of the probability that a given set of mitigating actions will have the intended result.

Concerns over the lack of attention to the uncertainty issue are heightened by several additional issues, mostly related to the issues of model calibration and subsequent "corroboration" (a term which I interpret as being intended to substitute for the more commonly used term "validation"). First, while effort was expended in calibrating the model for all 9 river segments using one year's (2000) worth of data, attempts to "corroborate", and thereby evaluate model performance independently, seem to have only been undertaken in a couple of upstream segments (i.e. those residing almost exclusively in

Oregon)<sup>1</sup>. None of the California segments appeared to undergo any type of validation/corroboration analysis (with the exception of the estuary, segment 9). Predictions based on water quality models, even the most advanced models parameterized with extensive data sets, are often highly divergent from observations, and without any evaluation of model performance, it difficult to place a high level of confidence in these modeled results. This would seem to be relevant given that one of the central themes in the analysis involves comparing model results from “current” conditions with the results of models designed to estimate “natural” background conditions. Furthermore, it appears that in some cases relatively small deviations between modeled estimates of current and natural conditions serve as the basis for a decision on the location and magnitude of a loading reduction. While the choice as to whether or not these models are accurate enough to reasonably support decisions on actions is a matter for policymakers to decide, I think that some quantification and presentation of the uncertainty associated with these estimates would greatly facilitate more informed decisions.

I am aware and sympathetic to the argument that academics think there is “never enough data”, but still believe that there may be opportunities to better convey the level of uncertainty in modeled estimates. Along those lines, it appears that the corroboration/validation efforts were limited by both data availability and the cost associated with doing additional modeling (explanation given Chapter 3, pg. 7). I do not know the relative roles that each played in the decision to forego the validation step, and of course if there are no data available to undertake additional modeling, that is one issue (although one that might be revisited). However, if data availability is not limiting, I would offer some suggestions.

If sufficient data on current conditions exists to reasonably validate the model for the lower (i.e. California) segments of the Klamath basin , a more rigorous quantitative approach to evaluating the confidence intervals associated with estimates of current conditions would allow for a more informed comparison of current and natural conditions. In addition, while historical data on “natural” conditions are not likely to be available, some attempt at a sensitivity analysis, including an identification of the most sensitive model inputs and an evaluation of the impacts that varying these inputs has on model estimates of water quality, would provide some sense of model limitations (as currently presented, at least in Figures 2.15 and 2.16, it appears that there is very little uncertainty in modeled natural conditions). In the event that data on current conditions in the lower segments (6-9) is lacking, such a sensitivity analysis could be undertaken here as well. Some justification for the ranges of input values selected would also be informative.

I might also suggest that if increased efforts are made to collect water quality data in the system, either as a part of this or subsequent efforts, some careful planning involving

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<sup>1</sup> The models of segments 1-5 appear, in some cases, to have undergone some sort of “validation” step , in which year 2002 conditions are estimated using a model calibrated with 2000 data, and these conditions then compared to observed data from 2002. Nonetheless, when comparisons of model results and observed data from 2002 are presented in Appendix 5 (e.g., Figures G-8, G-9 and G-10), they are labeled as “calibration results”, so I may have interpreted the term “corroboration” incorrectly.

consideration of a joint modeling and monitoring approach might be useful. Current advancements in the science of merging observations and modeling results in water quality can significantly reduce the costs of rigorously characterizing conditions in a river system (LoBuglio et al. 2007; Money et al. 2009) and might be worth investigating at some point.

The existing data seems to suggest that human activities are contributing to water quality impairment in the Klamath Basin. Nonetheless, the degree to which this impairment is occurring and the level to which current conditions deviate from natural conditions is very difficult to determine using modeling as a primary analytical tool. I understand that this may be all that is currently available, but believe that a more explicit treatment of the uncertainty associated with modeling results will provide decisionmakers with a more informed basis for making policy choices.

Let me reiterate that I find the models to be consistent with sound scientific principles, and the most up-to-date thinking on water quality models, the simple fact is that even state-of-the-art water quality models are not terribly accurate. And, while one could always take issue with individual assumptions or particular input values, I am not sure that one set of choices would necessarily be better than others. I do believe, however, that the lack of explicit attention to the uncertainty issue could leave the impression that these models are more accurate than they actually are. Consequently, a more concerted effort to evaluate and communicate the uncertainty inherent in these models would seem appropriate.

#### Specific Issues

##### **(1) Nutrient Allocations and chlorophyll-a, *Microsystis aeruginosa*, and microcystin numeric targets for Copco 1 and 2 and Iron Gate Reservoirs developed to control blue-green algae blooms, associated toxins, and protect recreation and cultural beneficial uses**

The use of chlorophyll-a as an indicator of algal growth, including *Microsystis aeruginosa*, and the accompanying microcystin seems supportable given the data presented in Figures 2.1-2.6. Similarly, the choices of target values for these three parameters seem reasonable. I am less sure of the nutrient allocations and whether the targets suggested can be fully supported by the evidence presented. There is very little effort directed toward characterizing the degree to which nutrient inputs contribute to increased algal growth. Model runs to determine algal and chlorophyll-a concentrations were undertaken for river segments upstream of the reservoirs, in particular segment 5, but in this case the models tended to substantially overpredict both, by several multiples in most cases (Figures H-17, H-24, H-31 and H-39). While the instream models are different from that (CE-QUAL) used to model the reservoirs, it does not provide a high degree of confidence that the nutrient input targets are an accurate indicator of the outcome in terms of reducing chlorophyll-a, *Microsystis aeruginosa*, and microcystin to desired levels.

Further downstream in Copco and Iron Gate reservoirs, observations of chlorophyll-a concentrations are used to calibrate the CE-QUAL model across a range of depths on several dates (Figures I-6 and J-6). Unfortunately, there is no validation exercise, and

the scale on the horizontal axis of the calibration figures is in mg/l (the standard for chlorophyll-a is measured in ug/l) making it difficult to determine the relative degree of accuracy inherent in the calibration exercises. As a result, the linkage between nutrient inputs and the biological endpoints of primary interest (i.e. chlorophyll-a, *Microcystis aeruginosa*, and microcystin) is unclear, and I would have liked to have seen a more explicit rendering of the uncertainties associated with these predictions.

With regard to the allocation of “zero nutrient loading from the reservoir bottom sediments”, I have a few questions. Is this intended to mean zero additional, *human-induced*, nutrient loading from the bottom sediments, or zero nutrient loading of any kind? If the latter, this seems a bit strange, as I would guess that even in the river’s natural state, or a condition in which the reservoir exists without human-induced nutrient loadings, that there are sure to be some natural nutrient additions to the system. Some of these are bound to be in a particulate form and make their way to the sediments where they would contribute some (non-zero) nutrient load on the water column. In either event, the concept of a “zero” allocation target is a difficult one to conceive of in any natural context, and even if it were possible, the evidence presented does not provide a high level of confidence that the biological endpoints will be reached.

## **(2) Temperature and dissolved oxygen allocations to Copco and Iron Gate Reservoirs developed to support salmonid beneficial uses.**

Dissolved oxygen levels and temperature are clearly linked, and the data and analyses on fish behavior makes a good case that raising D.O. and lowering temperature in the system will enhance fish survival and reproduction. The temperature model seems to calibrate reasonably well for the Copco and Iron Gate reservoirs (Figures I-1 and J-1), but some validation step would have been comforting. That said, I am not sure I understand how the targets for temperature will be met, as the thought that the difference in reservoir inflow and outflow in Copco and Iron Gate can be limited to (on average) 0.1 C and 0.3 C, respectively, seems very unlikely given the temperature data presented in Figures 4.7 and 4.8. The residence time in the reservoirs, and hence the longer exposure to sunlight and, particularly in summer, higher air temperatures would seem to make achieving this goal difficult, even with the understanding that dam releases often involve cooler water from the middle of the water column.

The concept of the “compliance lens”, while being new to me, is an interesting one and in theory could be quite useful, however, I am skeptical regarding the ability to design a thermal load allocation strategy that will reliably result in such a lens. While it is tempting to view reservoirs as entirely quiescent bodies of water, almost all have circulation patterns driven by wind, inflows, etc. The thought that such a large and complex natural system could be fine tuned to the degree necessary to consistently create a lens with the desired D.O. and temperature conditions strikes me as being very optimistic. Nonetheless, given the information presented in the report, if such a lens could be established, it would appear to offer a “home” to sensitive fish populations, provided they are capable of finding and making use of such regions, and assuming that no other factors (e.g., food availability) impact their ability to remain in them (I know

very little about fish behavior/biology, so I am not qualified to offer many useful comments on these issues).

Lastly, I am curious as to why climate change is not explored as a possible reason for increased reservoir and stream temperatures. Surely there is data available on air temperatures in the basin, and it would be relatively easy to look for trends in increasing mean, high and low values over time. If air temperatures have been increasing, particularly increased low temperatures at night (which seem to be where the biggest impacts are observed), this would appear to be an obvious contributor to increased water temperatures in the Klamath. These are certainly “human-induced” increases to thermal load, but local actions to combat these contributions would not likely be effective. As a result, some discussion of this issue, and an analysis of the size of climate change related contributions, if any, to those from other sources (return flows, altered channel dimensions, etc.) would seem to be important when developing mitigation strategies.

### **(3) Analysis of the effects of tributary stream flow rates on stream temperatures in the tributaries and Mainstem of the Klamath River.**

I hope I have not missed something in this area (and I believe I have exercised due diligence), however if I have not, it appears to me that there is insufficient data and/or evidence to support even general assessments of changes in the thermal conditions of the Klamath tributaries, or to evaluate actions that might mitigate any potential impairment. I understand that professional judgment will play a role in decisions on whether and how to regulate these systems, and that these decisions must often be made without the benefit of sufficient data to conclusively demonstrate that the proposed actions will work as intended. Nevertheless, in this case, the relative dearth of information makes it difficult for me to understand how there is a basis for any considered decisions.

The question of whether or not a thermal impairment exists in these tributaries, and as a result, in the mainstem of the Klamath itself, revolves primarily around a comparison of natural and current conditions. It appears that both sets of conditions are evaluated almost entirely on the basis of modeling results. I could find no evidence that models of temperature in these tributaries had been calibrated with actual observations, much less validated. The only data related to this question appeared to be in Figures 2.11 and 2.13, which show some data on mainstem temperatures at the points where the tributaries enter the mainstem, but do not provide enough information to make any determination of their potential impact. The model results are contingent on accurate information related to flow rates, channel morphology, runoff inputs, effective shade and a host of other factors for which very little current data appears to exist (information on what would constitute “natural” conditions is, of course, even more scarce). Previous modeling efforts are alluded to and seem to serve as a basis for the modeling exercises in this effort (Chap. 3, pg. 11), so maybe there was some data associated with them. If so, it would be nice to include some discussion of this. Even if a comprehensive set of accurate model inputs were available, however, I think it would be difficult to use these models to try to distinguish the relatively subtle changes in

stream temperature that would form the basis for a decision on whether or not the tributary were impaired (or whether the tributary contributed to the impairment of the mainstem of the river).

Section 3.3.3.2 of the Analytical Methods section describes a series of assumptions and modeling scenarios that suggest very little data on these systems exists (and no data is presented). The Scott River in particular seems to have been modeled with very little information other than some current flow data (Table 3.2). With regard to the other tributaries, the point is made that changes in effective shade and stream channel dimensions can have an impact on stream temperature, which is no doubt true, but the evidence that changes in either of these areas have taken place in the tributaries seems largely anecdotal. There is some vague mention of changes in land use and the effects that flooding may have had on stream channel width and riparian vegetation, but no data on this is presented (section 2.5.2.2). The subsequent analysis of the impacts of different levels of effective shade demonstrates that there could be an impact, but little evidence is provided to suggest that there actually has been a change in riparian vegetation. Similarly, a discussion in section 4.2.4.1 on the potential impacts of sediment load on temperature in the tributaries cites a higher peak stream temperature the year after a flood (on the basis of seven years of data) as evidence that sediment loads are a factor which seems very shaky. This is then followed up by a statement describing modeling results that suggest a doubling of stream width can increase temperatures 1-2 C, but there is no data presented to suggest that stream widening in any of the tributaries has occurred.

As with question (3) above, I also find myself wondering whether there have been trends toward increasing air temperatures in the Basin (i.e. climate change). This would be another area in which data certainly exists, and would seem important to explore when trying to identify potential sources of increased stream temperature.

I do not want to be overly harsh here, but unless there is substantially more data and analysis of this issue than has been presented in these documents, my opinion is that there is insufficient information to make any informed judgements.

#### **(4) Assessing the linkage between water quality and fish disease.**

I have read through these sections and the conclusions, based on my very meager knowledge in these areas, appear to be reasonable. That said, I have no background in the biology of fish or any other form of macrobiota, so I am not at all qualified to make judgments on the scientific basis for establishing linkages between water quality and fish disease. I would, however, suggest that Professor Hans Paerl at the University of North Carolina's Institute for Marine Sciences, would be someone capable of providing a knowledgeable review in this area or, at a minimum, could point toward other individuals with related expertise.

## References

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